

Contrastive Empiricism

I

Despite what Hegel may have said, syntheses have not been very successful in philosophical theorizing. Typically, what happens when you combine a thesis and an antithesis is that you get a mishmash, or maybe just a contradiction. For example, in the philosophy of mathematics, formalism says that mathematical truths are true in virtue of the way we manipulate symbols. Mathematical Platonism, on the other hand, holds that mathematical statements are made true by abstract objects that exist outside of space and time. What would a synthesis of these positions look like? Marks on paper are one thing, Platonic forms another. Compromise may be a good idea in politics, but it looks like a bad one in philosophy.

With some trepidation, I propose in this paper to go against this sound advice. Realism and empiricism have always been contradictory tendencies in the philosophy of science. The view I will sketch is a synthesis, which I call Contrastive Empiricism. Realism and empiricism are incompatible, so a synthesis that merely conjoined them would be a contradiction. Rather, I propose to isolate important elements in each and show that they combine harmoniously. I will leave behind what I regard as confusions and excesses. The result, I hope, will be neither contradiction nor mishmash.

II

Empiricism is fundamentally a thesis about *experience*. It has two parts. First, there is the idea that experience is necessary. Second, there is the thesis that experience suffices. Necessary and sufficient for what? Usually this blank is filled in with something like: knowledge of the world outside the mind. I will set the

This paper is dedicated to the memories of Geoffrey Joseph and Joan Kung—two colleagues and friends from whom I learned a lot. Each influenced the way my views on scientific realism have evolved. I will miss them both.

issue of knowledge to one side and instead will focus on the idea that experience plays a certain role in providing us with justified beliefs about the external world. Never mind what the connection is between justified belief and knowledge.

These two parts of empiricism have fared quite differently in the past 200 years or so. The idea that experience is necessary has largely lapsed into a truism. No one thinks that a priori reflection all by itself could lead to reasonable science. Later, I'll identify a version of this necessity thesis that is more controversial.

The other thesis—that experience somehow suffices—has been slammed pretty hard, at least since Kant. Percepts without concepts are blind. Or as we like to say now, there is no such thing as an observation language that is entirely theory-neutral. Although positivists like the Carnap of the *Aufbau* tried to show that this empiricist thesis could be made plausible, it is now generally regarded as mistaken or confused.

One vague though suggestive metaphor for what empiricism has always aimed at is this: our knowledge cannot *go beyond* experience. Pending further clarification, it is unclear exactly how this idea should be understood. But the basic thrust of this idea has also come in for criticism. The standard point is that pretty much everything we believe about the external world goes beyond experience. Even a simple everyday claim about the commonsense characteristics of the physical objects in my environment goes beyond the experiences I have had or can ever hope to have. A consistent empiricism, so this familiar line of criticism maintains, ultimately leads to a solipsism of the present moment.

How should realism be understood? There are many realisms. Realism is often described as a thesis about what truth is or as a thesis about what is true. Neither of these is the realism I will address.

Realism as a view about the nature of truth is a semantical thesis; a realist interpretation of a set of sentences will claim that those sentences are true or false independently of human thought and language.¹ The sentences are said to describe a mind-independent reality and to depend for their truth values on it. The standard opponent of this semantical thesis has been verificationism, which either rejects the notion of truth or reinterprets it so that truth and falsity are said to depend on us in some way. This semantical issue will not concern me further. The issue between realism and empiricism that I want to examine concedes that truth is to be understood realistically.

Realism is sometimes described as a thesis about how we should interpret the best scientific theories we now have. We should regard them as true and not simply as useful predictive devices that tell us nothing about an unobservable reality. There really are genes and quarks, so this sort of realist says.

Putnam (1978) has challenged this realist position by claiming that our present theories will probably go the way of all previous theories—future science will find them to be false. He uses this inductive argument to say that we are naive if we

regard current science as true. Realism of this sort, he claims, is predicated on the unscientific expectation that the future will not resemble the past.

Putnam's argument strikes me as overstated. I don't think that *all* previous scientific theories have been found to be false in every detail. Rather, historical change has preserved some elements and abandoned others. Nevertheless, I think his skepticism about labeling all our best current theories as true is well taken. A realist in my sense may decline to say that this or that present theory is true.

Realism, in the sense at issue here, is not a thesis about what truth is; nor is it a thesis about what is true. Rather, it is a thesis about the goals of science. This is the realism that Van Fraassen (1980) singled out for criticism. Science properly aims to identify true theories about the world. Realists may refuse to assert that this or that current theory is true, though they perhaps will want to say that some theories are our current best guesses as to what is true.

What would it be to reject this thesis about the proper goal of science? Empiricism, in Van Fraassen's sense, holds that the goal of science is to say which theories are empirically adequate. Roughly, empirical adequacy consists in making predictions that are borne out in experience.

How could the search for truth and the search for empirical adequacy constitute distinct goals? Consider two theories that are *empirically equivalent*, but which are not merely notational variants of each other. Though they disagree about unobservables, they have precisely the same consequences for what our experience will be like. Not only are the theories both consistent with all the observations actually obtained to date; in addition they do not disagree about any possible observation. A realist will think that it is an appropriate scientific question to ask which theory is true; an empiricist will deny that science can or should decide this question.

The idea of empirical equivalence has had a long history. Descartes's evil-demon hypothesis was constructed to be empirically equivalent with what I'll call "normal" hypotheses describing the physical constitution of the world outside the mind. In the last hundred years, the idea of empirical equivalence has played a central role in the philosophy of physics. Mach, Poincaré, Reichenbach, and their intellectual heirs have used this idea to press foundational questions about the geometry of space and about the existence of absolute space.

In problems of this sort, realists appeal to criteria that discriminate between the two competing hypotheses and claim that those criteria are scientifically legitimate. Perhaps we should reject the evil-demon hypothesis and accept the normal hypothesis because the latter is more parsimonious, or because the former postulates the existence of an unverifiable entity. The same has been said about the existence of absolute space and the existence of universal forces, which Reichenbach (1958) introduced to play the role of an evil demon in the problem of geometric conventionalism. Realists argue that criteria of this sort provide legitimate grounds for claiming that some theories are true and others are false. Empiricists

disagree, arguing that these criteria are merely “aesthetic” or “pragmatic” and should not be taken as a ground for attributing truth values.²

I mention parsimony and verifiability simply as examples. Realists may choose to describe the criteria they wish to invoke in an entirely different way. The point is that realists claim that scientific reasoning is *powerful* in a way that empiricists deny.³

III

The development of empiricism has been guided by the following conditional: If our knowledge cannot go beyond experience, then it should be possible to delimit (i) a set of *propositions* that can be known and (ii) a set of *methods* that are legitimate for inferring what is true. For this reason empiricists have felt compelled (i) to draw a distinction between observation statements and theoretical ones and (ii) to develop a picture of the scientific method whereby the truth of theoretical statements can never be inferred from a set of observational premises.

The result is generally thought to have been a two-part disaster. The theory/observation distinction has been drawn in different ways. But each of them, I think, has either been too vague to be useful, or, if clear, has been epistemologically arbitrary. Maxwell (1962) and Hempel (1965), among many others, asked why the size of an object should determine whether it is possible to obtain reasonable knowledge about it. Apple seeds are observable by the naked eye, but genes are not. Hempel asked “So what?”—a question that empiricism’s critics have continued to press.

Empiricist theories of inference have fared no better. If empiricists are to block theoretical conclusions from being drawn from observational premises, they must narrowly limit the rules of inference that science is permitted to use. Deduction receives the empiricist seal of approval, and maybe restricted forms of “simple induction” will do so as well. But there are scientific arguments from observational premises to observational conclusions that do not conform to such narrow strictures. Rather, they seem to require something philosophers have liked to call “abduction”—inference to the best explanation.⁴ However, once these are admitted to the empiricist’s organon of methods, the empiricist’s strictures dissolve. The point is that inference to the best explanation also seems to allow theoretical conclusions to be drawn from observational premises. This now-standard argument against empiricism recurs so often that it deserves a name; I call it *the garden-path argument*.

These familiar problems affect Van Fraassen’s (1980) constructive empiricism just as much as they plague earlier empiricisms. Van Fraassen says that it is appropriate for science to reach a verdict on the truth value of statements that are strictly about observable entities. But when a statement says something about unobservable entities, no conclusion about its truth value should be drawn. In this

case, the scientist should consider only whether the statement is empirically adequate.

Van Fraassen takes various facts about our biology to delimit what sorts of entities are observable. Observable means observable *by us*. But the question then arises as to why no legitimate forms of scientific inference can take us from premises about observables to conclusions about unobservables. As with previous empiricisms, constructive empiricism seems to impose an arbitrary limit on the kinds of inferences it deems legitimate.

There is an additional problem with Van Fraassen's approach. It concerns the concept of aboutness. The appropriate scientific attitude we should take to a statement is said to depend on what that statement is about. But what is aboutness? I see no reason to deny that the statement "All apples are green" is about everything in the universe; it says that every object is green if it is an apple. If the universe contains unobservable entities, then the generalization is about them as well. Pending some alternative interpretation of "aboutness," constructive empiricism seems to say that science should not form opinions about the truth value of any generalization (Musgrave 1985, 208; Sober 1985).

Modern empiricism has frequently been plagued by semantical problems. Carnap tried to divide theoretical from observational statements by a verificationist theory of meaning. Van Fraassen abandons this empiricist semantics, but his theory is undermined by a semantical difficulty all the same. It is aboutness, not verificationism, that causes the problem.

Realism appears strongest when it deploys criticisms of empiricism of the kinds just mentioned. The best defense is a good offense. But when one looks at the positive arguments that realists have advanced, their position looks more vulnerable. Indeed, the problems become most glaring when the issue of empirically equivalent theories is brought to the fore.

Before Putnam lapsed from the realist straight and narrow, he sketched an argument for realism that struck many philosophers as very powerful. It is encapsulated in Putnam's (1975) remark that "realism is the only philosophy that doesn't make the success of science a miracle." I now want to consider this miracle argument for realism.

The idea is this. Suppose a theory *T* is quite accurate in the predictions it generates. This is something on which the realist and the empiricist can agree. The question is then: *Why* is the theory successful? What explains the theory's predictive accuracy?

The miracle argument seeks to show that the hypothesis that the theory is true (or approximately true) is the best explanation of why the theory is predictively accurate. If the theory postulates unobservables, then the theory's predictive success is best explained by the hypothesis that the entities postulated by the theory really exist and their characteristics are roughly as the theory says they are. Here

is an example of an abductive argument that leads from observational premises to a nonobservational conclusion. The empiricist must block this argument.

Fine (1984, 84–85) has argued that the realist cannot employ abductive arguments of this sort, since they are question begging. The issue, he says, is precisely whether abduction is legitimate. Boyd (1984, 67) rejects this criticism. He claims that scientists use abductive arguments, and so it is quite permissible for a philosopher to use abduction to defend a philosophical thesis about science.

My assessment of the miracle argument differs from both Fine's and Boyd's. I have no quarrel with philosophical abductive arguments, as long as they conform to the standards used in science. The problem with the miracle argument is not that it is abductive, but that it is a very weak abductive argument.

When scientists wish to assess the credentials of an explanatory hypothesis, a fundamental question will be: What are the alternative hypotheses that compete with the one in which you are really interested? This is the idea that theory testing is a contrastive activity. To test a theory *T* is to test it against at least one competing theory *T'*.

The miracle argument fails to specify what the set of competing hypotheses is supposed to be. The hypothesis of interest is that *T* is true or approximately true in its description of unobservables. If the problem is to choose between *T* and *T'*, where *T'* is a theory that is not predictively equivalent with *T*, then the miracle argument might make sense. That is, if the choice is between the following two conjectures, there clearly can be good scientific evidence favoring the first:

(One) *T* is true or approximately true.

(Two) *T'* is true or approximately true.

But if I vary the contrasting alternative, matters change. What scientific evidence can be offered for favoring hypothesis (One) over the following competitor:

(Three) *T* is empirically adequate, though false.

If (Three) were true, it would not be surprising that *T* is predictively successful.

So what becomes of the thesis that realism is the only hypothesis that doesn't make the success of science a miracle? Strictly speaking, it is false. A realist interpretation of the theory *T* is given by (One); if true, it would explain what we observe—that *T* is predictively successful. But the same holds of (Three); if it were true, that also would explain the predictive success of *T*.

Notice that my critique of the miracle argument does not proceed by artificially limiting science to a discussion of observables. Nor does it reject the legitimacy of abduction. Both *T* and *T'* may talk about unobservables; and the choice between hypotheses (One) and (Two) may be an unproblematic case of inference to the best explanation. The criticism just sketched differs from the empiricist's

standard position. Rather, it is characteristic of the view that I call Contrastive Empiricism.

It may be objected that hypothesis (*Three*) is really no explanation at all of the predictive success that *T* has enjoyed. If Holmes finds a corpse outside of 221B Baker Street, the hypothesis that Moriarty is the murderer is one possible explanation. But is it an explanation to assert that Moriarty is innocent, though the crime looks just as it would have if Moriarty had done the dirty deed? Does this remark explain why the murder took place?

This question is a subtle one for the theory of explanation. Perhaps there are occasions in which saying that *T* is not the explanation may itself be an explanation; perhaps not. What I wish to argue is that this point is irrelevant to the issue of whether the miracle argument is successful. The question before us is first and foremost a question about *confirmation*. We want to know whether the predictive accuracy of theory *T* is good evidence that *T* is true or approximately true. The issue of explanation matters here only insofar as explanation affects confirmation. My view is that loose talk about abduction has brought these two ideas closer together than they deserve to be.

I'll use Bayes's theorem to illustrate what I have in mind. This theorem says that the probability, $Pr(H/O)$, of a hypothesis (*H*) in the light of the observations (*O*) is a function of three other probabilities:

$$Pr(H/O) = Pr(O/H)Pr(H)/Pr(O).$$

We wish to compare the probability of hypothesis (*One*) and hypothesis (*Three*), given that theory *T* has been predictively successful (*O*). (*One*) is more probable than (*Three*), in the light of this observation, precisely when:

$$Pr(O/One)Pr(One) > Pr(O/Three)Pr(Three).$$

The conditional probabilities in this last expression are called *likelihoods*: the likelihood of a hypothesis is the probability it confers on the observations. Don't confuse this quantity with the hypothesis's posterior probability, which is the probability that the observations confer on the hypothesis. So whether the above inequality is true depends on the likelihoods and the prior probabilities of hypotheses (*One*) and (*Three*).

In this Bayesian format,⁵ it is likelihood that represents the ability of the hypothesis to explain the observations. The question of whether the hypothesis explains the observations is interpreted to mean: how probable are the observations, if the hypothesis is true? A hypothesis with a small likelihood says that the observations are very improbable—that it is almost a “miracle,” so to speak, that they occurred. Understood in this way, hypotheses (*One*) and (*Three*) are equally explanatory, since they confer the same probability on the observations.

It may be replied, with some justice, that likelihood does not fully capture the idea of explanatory power. Indeed, there are reasons independent of the problem

of comparing hypotheses (*One*) and (*Three*) for thinking this. For example, two correlated effects of a common cause may make each other quite probable; given the presence of one effect, it may be more likely to infer that the other is present than that it is absent. Yet neither of the correlates explains the other. All this may be true, but my question, then, is this: When explanatory power diverges from likelihood, why think that explanatory power is relevant to confirmation? In the present case, let us grant that (*One*) is more explanatory than (*Three*). Why is this evidence that (*One*) is more plausible than (*Three*)?

Again, I want to emphasize that my criticism does not reject the idea of inference to the best explanation. Theoretical hypotheses about unobservables—like (*One*) and (*Two*)—have likelihoods. Inference to the best explanation should take those likelihoods into account. The problem, though, is that empirically equivalent theories have identical likelihoods.

I am reluctant to take “explanatory power” as an unanalyzed primitive that conveniently has just the characteristics that realists need if they are to justify their pet discriminations between pairs of empirically equivalent theories. Perhaps a non-Bayesian confirmation theory can make good on this realist idea. I don’t know of any proposal that does the trick. So I am reluctant to allow the miracle argument to proceed as a resolution of the problem of choosing between (*One*) and (*Three*).

In the Bayesian inequality stated before, there are other elements besides likelihoods. In addition, there are the prior probabilities of hypotheses (*One*) and (*Three*). If (*One*) were a priori more probable than (*Three*), that would help the realist, although it would be wrong to say that the empirical accuracy of theory *T* was doing any work. If the likelihoods are the same, then the observation is idle. Perhaps we shouldn’t call this the miracle argument at all; it isn’t that realism is a better explanation of what we observe. Rather, the idea now is that a realist interpretation of a theory is a priori more probable than the alternative.

I am at a loss to see how this idea can be parlayed into a convincing argument for realism. What do these prior probabilities mean? If they are just subjective degrees of belief that some agent happens to assign, we are simply saying that this agent favors realism before any observations have been made. This is hardly an argument for realism, since another agent could have just the opposite inclination.

If prior probabilities are to be used in an argument, it must be shown why hypothesis (*One*) *should* be assigned a higher prior than (*Three*). I know of no way of doing this, though perhaps an a priori argument for realism will someday be invented. Let me emphasize, however, that this is worlds away from Putnam’s a posteriori miracle argument. That argument is defective, if explanatory power goes by likelihoods; or it is entirely unclear what the argument says, if explanatory power is to be understood in some other way.⁶

I began this section by rehearsing the standard criticism that empiricism takes

an overly narrow view of the scope and limits of scientific inference. Conclusions about unobservables can be blocked only by drastically restricting inferences in a way that seems entirely artificial. But the present discussion of the miracle argument suggests that realism errs in the opposite direction. The idea of inference to the best explanation presupposed by the miracle argument licenses too much, if a roughly Bayesian idea of confirmation is used.

The empiricist wants to show that there is an important sense in which our knowledge cannot go beyond experience. The realist wants to show that our ability to know about quarks is every bit as strong as our ability to find out about tables. The empiricist idea runs into trouble when it artificially limits the power of scientific inference. The realist idea runs into trouble when it artificially inflates that power. It now is time to see how the defensible kernel of each position can be formulated as a single position, one that avoids the excesses of each.

IV

I mentioned before that theory testing is a contrastive activity. If you want to test a theory *T*, you must specify a range of alternative theories—you must say what you want to test *T* against.

There is a trivial reading of this thesis that I do not intend. To find out if *T* is plausible is simply to find out if *T* is more plausible than *not-T*. I have something more in mind: there are various contrasting alternatives that might be considered. If *T* is to be tested against *T'*, one set of observations may be pertinent; but if *T* is to be tested against *T''*, a different set of observations may be needed. By varying the contrasting alternatives, we formulate genuinely different testing problems.

An analogous point has been made about the idea of explanation.⁷ To explain why a proposition *P* is true, we must explain why *P*, rather than some contrasting alternative *Q*, is true (Dretske 1973; Garfinkel 1981; Van Fraassen 1980; Sober 1986; but see Salmon 1984 for criticisms). This thesis is nontrivial, since varying the contrasting proposition *Q* poses different explanatory problems.

A nice example, due to Garfinkel, concerns the bank robber Willi Sutton. A priest once asked Willi why he, Willi, robbed banks. Willi answered that that was where the money was. The priest wanted to know why Willi robbed rather than not robbing. Willi took the question to be why he robbed banks rather than candy stores.

The choice of a contrasting alternative helps delimit what sort of propositions may be inserted into an answer to a why-question. If you ask why Willi robbed banks rather than candy stores, you may include in your answer the assumption that Willi was going to rob something. However, if you ask why Willi indulged in robbing instead of avoiding that activity, you cannot include the assumption that Willi was going to rob something.

Why is it legitimate to insert the statement that Willi was going to rob something into the answer to the first question, but not into the second? Consider the question "Why *P* rather than *Q*?" I claim that statements implied by both *P* and *Q* are insertable. The question presupposes the truth of such statements, so they may be assumed in the answer. On the other hand, the implications that *P* has that *Q* does not cannot be inserted into an answer. It is matters such as these that are at issue, so assuming them in the answer would be question begging.

I have described a sufficient condition for insertability and a sufficient condition for noninsertability. I will not propose a complete account by specifying a necessary and sufficient condition. The modest point of importance here is that the formulation of an explanatory why-question often excludes a statement from being inserted into an answer.

I turn now from explanation to confirmation. Instead of asking "Why *P* rather than *Q*?" I want to consider the question "Why *think P* rather than *Q*?" This is a request for evidence or for an argument showing that *P* is more plausible than *Q*. I want to claim here that confirmatory why-questions often exclude some statements from being insertable.

A statement *S* will not be insertable into an answer to such questions, if it is not possible to know that *S* is true without already knowing that *P* is more plausible than *Q*. What is requested by the question is an independent reason, not a question-begging one.

As in the case of explanation, a statement may be insertable into the answer to one confirmatory why-question without being insertable into the answer to another. One way to change a question so that an answer insertable before is no longer so, is by a procedure I'll call *absorption*. Suppose I ask why Willi led a life of crime rather than going straight? You might answer by citing Willi's tormented adolescence. But if I absorb this answer into the question, I thereby obtain a new question, which cannot be answered by your previous remark. Suppose I ask: Why did Willi have a tormented adolescence and then lead a life of crime, as opposed to having an idyllic adolescence followed by a law-abiding adulthood? The assertion that Willi had a tormented adolescence is not insertable into an answer to this new question.

It is just this strategy of absorption that philosophers have used to generate skeptical puzzles about empirically equivalent theories. The question "Why think *P* rather than *Q*?" may have *O* as an answer. But *O* cannot be inserted into an answer to a new question: "Why think that *P* and *O* are true, rather than *Q* and *not-O*?" Reichenbach (1958) argued that if I assume a normal physics devoid of universal forces, I can develop experimental evidence for thinking that space is non-Euclidean rather than Euclidean. But if I absorb the assumptions about a normal physics into my question, I obtain a new question that is not, according to Reichenbach, empirically decidable: the reason, he claimed, is that the conjunction of the non-Euclidean hypothesis and normal physics is empirically equivalent

to the conjunction of the Euclidean hypothesis and a physics that postulates universal forces.

As noted earlier, empiricists have always maintained that it is not possible to say that one theory has a better claim to be regarded as true than another, if the two are empirically equivalent. Such discriminatory why-questions, the empiricist claims, are unanswerable.

Empiricists have defended this view by claiming that there is a privileged set of statements—formulated in the so-called observation language. Sentences in this special class were supposed to have the following feature—ascertaining their truth required no theoretical information whatever. The empiricist claim then was made that a discrimination can be made between two theories only if they make different predictions about what will be true in this class of sentences.

The standard criticism of this idea was developed by claiming that the distinction between theory and observation is not absolute. The very statements that on some occasions provide independent answers to confirmatory why-questions at other times provide only question-begging answers. A statement that counts as an observation statement in one context can become the hypothesis under test in another. The observation/theoretical distinction, so this criticism of empiricism maintained, is context relative and pragmatic.

The standard empiricist claim about observation has a quantifier order worth noting:

- (EA) There exists a set of observation statements, such that, for any two theories T and T' , if it is possible to say that T is more plausible than T' , then this will be because T and T' make incompatible predictions as to which members of that set are true.

A weaker thesis, which avoids an absolute distinction between theory and observation, has a different quantifier order:

- (AE) For any two theories T and T' , if it is possible to say that T is more plausible than T' , then this will be because there exists a set of observation statements such that T and T' make incompatible predictions as to which members of that set are true.

(EA) is committed to an absolute distinction between theory and observation; the required distinction is absolute because what counts as an observation statement is invariant over the set of testing problems. (AE) is not committed to this thesis, because it is compatible with the idea that what counts as an observation is relative to the testing problem considered. Contrastive Empiricism maintains that (AE), rather than the stronger thesis (EA), is correct. What counts as an observation in a given test situation should provide non-question-begging evidence for discriminating between the competing hypotheses.

Contrastive Empiricism makes use of the concept of an observation, as does

the very formulation of the problem of empirically equivalent theories, which, recall, is a problem that both realists and empiricists want to solve. I have already mentioned that what counts as an observation may vary from one testing problem to another. But more must be said about what an observation is. I won't attempt to fully clarify this concept here, but again, will content myself with a sufficient condition for empirical equivalence, one with a Quinean cast. Two theories are empirically equivalent if the one predicts the same physical stimulations to an agent's sensory surfaces as the other one does. Observational equivalence is vouchsafed by identity of sensory input.

Both (EA) and (AE) mark the special role of experience in terms of a partition of propositions. The scientist testing a pair of theories is supposed to be able to identify a class of sentences in which the so-called observation reports can be formulated. But the empiricist's point about empirically equivalent hypotheses can be made in a quite different way. Consider an analogy: When your telephone rings, that is evidence that someone has dialed your number. But the ringing of the phone when I dial your number is physically indistinguishable from the ringing that would occur if anyone else did the dialing. This is an empirical truth that can be substantiated by investigating the physical channel. The proximal state fails to uniquely determine its distal cause.⁸ I don't need to invoke a special class of protocol statements and claim that they have some special epistemological status to make this simple point. Still less does the *telephone* need to be able to isolate a special class of sentences in which it can record its own physical state.

The idea of empirically equivalent hypotheses is parallel, though, of course, more general. It is basically the idea that the proximal state of the whole sentient organism, both now and in the future, fails to uniquely determine its distal cause. Whether two hypotheses are empirically equivalent is a question about the sensory channels by which distal causes can have proximal experiential effects. What engineers can tell us about telephones, psychologists will eventually be able to tell us about human beings. I don't think that the idea of empirical equivalence requires any untenable dualisms.⁹

The main departure of this "engineering" approach to the concept of empirical equivalence from earlier "linguistic" formulations is this: In the earlier version, the scientist is viewed as thinking about the world by deploying a certain *language*. The idea of empirical equivalence is then introduced by identifying a set of sentences within that very language; two theories are then said to be empirically equivalent if they make the same predictions concerning the truth of sentences in the privileged class. In the engineering version, we can talk about two theories being empirically equivalent for a given organism (or device) without supposing that the theories are formulable within the organism's language and without supposing that the organism has a language within which the experiential content of the observation is represented without theoretical contamination.¹⁰ It's

the sensory state of the organism that matters for the engineering concept, not some special class of statements that the organism formulates.

It is not to be denied that the theories that scientists standardly wish to test do not, by themselves, imply anything about the observations they will make. If neither of two theories has observational implications, then it is only in an uninteresting and vacuous sense that they are empirically equivalent. But what cannot be said of the part can be said of the whole. I take it that two, perhaps large, conjunctions of theoretical claims (including what philosophers like to call auxiliary assumptions) can have observational implications. And what is more, it sometimes can happen that two largish conjunctions can be empirically equivalent. This, I think, is what Descartes wanted to consider when he formulated his evil-demon hypothesis and what Reichenbach had in mind by his conjunction of a physics of universal forces and a geometry. Maybe these hypotheses were short on details, but I do not doubt that there are pairs of empirically equivalent theories. It is about such pairs that empiricism and realism disagree.¹¹

The main departure that Contrastive Empiricism makes from previous Empiricisms, including both Logical Empiricism and Constructive Empiricism, is that it is about *problems*, not *propositions*. Previous empiricisms, as I've said, have tried to discriminate one set of statements from another. Van Fraassen, like earlier empiricists, wants to say that science ought to treat some statements differently from others. Contrastive empiricism draws no such distinction. Rather, it states that science is not in the business of discriminating between empirically equivalent hypotheses.

For example, previous empiricisms have wanted to identify a difference between the following two sentences:

(X1) There is a printed page before me.

(Y1) Space-time is curved.

I draw no such distinction between these *propositions*. Rather, my suggestion is that there is an important similarity between two *problems*. There is the problem of discriminating between (X1) and (X2). And there is the problem of discriminating between (Y1) and (Y2):

(X2) There is no printed page before me; rather, an evil demon makes it seem as if there is one there.

(Y2) Space-time is not curved; rather, a universal force makes it seem as if it is curved.

According to Contrastive Empiricism, neither of these problems (when formulated with due care) is soluble.

Although Contrastive Empiricism embodies one part of the empiricist view that knowledge cannot go beyond experience, there is nonetheless an important realist element in this view. Hypotheses about the curvature of space-time may

be as testable as hypotheses about one's familiar everyday surroundings. (*X1*) can be distinguished from a variety of empirically nonequivalent alternatives, by familiar sensory means. (*Y1*) can be distinguished from a variety of empirically nonequivalent alternatives, by more recondite, though no less legitimate, theoretical means.

Contrastive Empiricism gives abduction its due. But when the explanations under test are empirically equivalent, it concedes that no difference in likelihood will be found. If we use a rough Bayesian format and claim that there is nonetheless a difference in plausibility between (*X1*) and (*X2*), or between (*Y1*) and (*Y2*), we therefore must be willing to say that there is a difference in priors. But where could this difference come from? Contrastive Empiricism claims that no such difference can be defended.

In less philosophically weighty problems of Bayesian inference, two hypotheses may have identical likelihoods, but differ in their prior probabilities for reasons that can be defended by appeal to experience. To use an old standby, if I sample at random from emeralds that exist in 1988 and find that each is green, then, relative to this observation, the following two hypotheses have identical likelihoods:

(*H1*) All emeralds are green.

(*H2*) All emeralds are green until the year 2000, but after that they turn blue.

In spite of this, I may have an empirical theory about minerals (developed before I examined even one emerald) that tells me that emerald color is very probably stable. This theory allows me to assign (*H1*) a higher prior than (*H2*).¹²

Contrastive Empiricism is not the truism that the likelihoods of empirically equivalent theories do not differ. Rather, it additionally claims that no defensible reason can be given for assigning empirically equivalent theories different priors. A pair of empirically equivalent hypotheses differs from the (*H1*)-(*H2*) pair in just this respect.

This is not to deny that human beings look askance at evil demons and their ilk. We do assume that they are implausible. In a sense, we assign them very low priors, so that even when their likelihoods are as high as the likelihoods of more "normal" sounding hypotheses, we still can say that normal hypotheses are more probable than bizarre evil-demon hypotheses in the light of what we observe. This is how we are, to be sure. But I cannot see a rational justification for thinking about the world in this way. I cannot see that we have any non-question-begging evidence on this issue. Maybe Hume was right that the combination of naturalism and skepticism has much to recommend it.

What does Contrastive Empiricism say about the principles that realists have liked to emphasize? Can't we appeal to simplicity and parsimony as reasons for rejecting evil demons and the like? Won't such considerations count as objective, since they also figure in more mundane hypothesis testing, where the candidates

are not empirically equivalent? Simplicity seems to favor (*H1*) over (*H2*) when both are consistent with the observations; so won't simplicity also favor (*X1*) over (*X2*) and (*Y1*) over (*Y2*)? Here the "garden-path argument" threatens to undermine Contrastive Empiricism. If appeals to parsimony/simplicity are permissible when the problem is to discriminate between empirically *nonequivalent* hypotheses, how can such appeals be illegitimate when the problem is to discriminate between hypotheses that are empirically equivalent?

Space does not permit me to discuss this issue very much. My view is that philosophers have hypostatized the principle of parsimony. There is no such thing. Rather, I think that when scientists appeal to parsimony, they are making specific background assumptions about the inference problem at hand. There is no abstract and general principle of parsimony, which spans all scientific disciplines like some abductive analog to *modus ponens*. When scientists draw a smooth curve through data points, to use a standard example, they do not do this because smooth curves are simpler than bumpy ones; rather, their preference for curves in one class rather than another rests on specific assumptions about the kind of process they are modeling.

Let me give an example that illustrates what I have in mind. Charles Lyell defended the idea of uniformitarianism in geology. He argued for this view by claiming that a principle of uniformity was a first principle of scientific inference, and that his opponents were not being scientific. However, if you look carefully at what Lyell was doing, you will see that "uniformitarianism" was a very specific theory about the Earth's history. Considered as a substantive doctrine, it is simply not true that uniformitarianism's rivals must be in violation of any first principle of scientific inference (Rudwick 1970; Gould 1985). On the other hand, if one abstracts away from the geological subject matter in the hope of identifying a suitably presuppositionless principle of simplicity or uniformity, what one obtains is a principle that has no implications whatever about whether Lyell's theory was more plausible than the alternatives.

Other examples of this sort could be enumerated. It has recently been popular for biologists to argue that group selection hypotheses should be rejected because they are unparsimonious. Those arguments, I think, are either totally without merit, or implicitly assume that the preconditions for certain kinds of evolutionary processes are rarely satisfied in nature. If parsimony is just abstract numerology, it is meaningless; if it really joins the issue, it does so by making an empirical claim about how evolution proceeds (Sober 1984). The evolutionary problem of phylogenetic inference affords another example: it is an influential biological idea that parsimony can be justified as a principle of phylogenetic inference without requiring any substantive assumptions about how the evolutionary process proceeds. Again, I think this view is mistaken; see Sober (1988) for details.

I grant that a few examples do not a general argument make. I also grant that the three examples I have cited do not involve choosing between empirically

equivalent theories. Could one grant my point that appeals to parsimony and simplicity involve contingent assumptions when the competing hypotheses are not empirically equivalent, but maintain that parsimony and simplicity are entirely presuppositionless when the choice is between empirically equivalent theories?

I find this view implausible. It also strikes me as pie-in-the-sky. Within a broadly Bayesian framework, it seems clear that prior probabilities are not obtainable a priori.¹³ If a plausible non-Bayesian confirmation theory can be developed that says differently, I would like to see it. Although I grant that our understanding of nondeductive inference is far from complete, I simply do not believe that the kind of confirmation theory that realism requires will be forthcoming.¹⁴

In "Empiricism, Semantics, and Ontology," Carnap (1950) introduced a distinction between internal and external *questions*, which he spelled out by distinguishing one class of *propositions* from another. Quine (1951) and others took issue with this absolute theory/observation distinction, and the rest is history. With Carnap, I believe that the idea that there are two kinds of questions is right; but unlike Carnap, I do not think this notion requires a verificationist semantics or an absolute distinction between observational propositions and theoretical ones.

Contrastive Empiricism reconciles the realist idea that we can have knowledge about unobservables with the empiricist idea that knowledge cannot go beyond experience. The view derives its realist credentials from the fact that it imposes no restrictions on the vocabulary that may figure in testable *propositions*; but it retains an important empiricist element in its claim that science cannot solve discrimination *problems* in which experience makes no difference. Again, the chief innovation of this version of empiricism is its focus on problems, not propositions.¹⁵

Whether Contrastive Empiricism is more plausible than the thesis and antithesis from which it is fashioned turns on epistemological issues that I have not been able to fully address here. I hope, however, to have at least put a new contrastive why-question on the table: the debate between realism and previous empiricisms – whether of the Logical or the Constructive variety – needs to be enlarged. Detailed work on the theory of hypothesis testing will show whether Contrastive Empiricism is more plausible than the philosophical hypotheses with which it competes.

Notes

1. It is the notion of independence deployed in this realist thesis that I try to clarify in Sober (1982).

2. Reichenbach (1938) is a classic example of this empiricist position.

3. I construe realism and empiricism as theses about how theories should be judged for their plausibility; neither thesis is committed to the claim that scientists *accept* and *reject* the hypotheses they assess. For one view of this controversial matter, see Jeffrey (1956).

4. An example: when biologists argue that the current distribution of living things is evidence that the continents probably were in contact long ago, the argument is not an induction from a sample to a containing population. Biologists did not survey a set of similar planets and see that continental drift accompanied all or most biogeographic distributions of a certain kind, and then conclude that the biogeographic distribution observed here on Earth was probably due to continental drift as well. The inference goes from an observed effect to an unobserved cause. Although the hypothesis that the continents drifted apart is not known by "direct" observation, empiricists nonetheless count it as an observation statement; if we had been present and had waited around for long enough, we could have observed continental drift. So inferences with observational conclusions often require a mode of inference that is neither deductive nor inductive.

5. It will become clear later that this approach is Bayesian only in the sense that it uses Bayes's theorem; the more distinctively Bayesian idea that hypotheses always have prior probabilities is not part of what I have in mind here.

6. Boyd (1980; 1984) advances a form of the miracle argument in which the fact to be explained is the reliability of the scientific method, rather than, as here, the reliability of a given theory. Boyd's version might be viewed as a diachronic analog of the synchronic argument I have discussed. My view is that the diachronic argument faces basically the same difficulties as the synchronic version.

7. The importance of contrasting alternatives has also been explored in connection with the problem of defining what knowledge is; see Johnsen (1987) for discussion and references to the literature.

8. Of course, a probabilistic formulation can be given to this idea: The probability of the phone's sounding a certain way, given that I dial your number, is precisely the same as the probability of its sounding that way if someone else does the dialing.

9. Van Fraassen (1980) rightly emphasizes that whether something is observable is a matter for science, not armchair philosophy, to settle. However, Van Fraassen also claims that an entity whose detection requires instrumentation should not count as "observable;" with Wilson (1984), I find this restriction arbitrary, however much it may accord with ordinary usage (Sober 1985).

10. Skyrms (1984, 117) similarly argues that what counts as an "observation might have a precise description not at the level of the language of our conscious thought but only at the level of the language of the optic nerve."

11. Although Wilson (1984) is properly skeptical about the empirical equivalence of some theory pairs that philosophers have taken to be related in this way, he nonetheless grants that there are such pairs; he cites results due to Glymour and Malament as providing cases in point.

12. There is a more global form of inductive skepticism that blocks this way of discriminating between *(H1)* and *(H2)*. If *all* empirical propositions—except those about one's current observations and memory traces—are called into question, what grounds are there for preferring *(H1)* to *(H2)*? This is the "theoretically barren" context in which Hempel (1965) posed his raven paradox. My view is that this skeptical challenge cannot be answered—only in the context of a background theory do observations have evidential meaning (Good 1967; Rosenkrantz 1977; Sober 1988).

13. But see Rosenkrantz (1977) for dissenting arguments.

14. This Bayesian approach to the conflict between realism and empiricism is very much in the spirit of Skyrms's (1984) pragmatic empiricism. Skyrms's main focus is on the idea of confirmation, which the Bayesian understands in terms of a comparison between the posterior and prior probabilities of a hypothesis; my focus has been on the idea of hypothesis testing, which is understood in terms of a comparison of two posterior probabilities. This difference in emphasis aside, we agree that empiricism should not be understood as a semantic thesis; nor should it claim that hypotheses about unobservables cannot be confirmed or tested.

15. Fine (1984) also proposes a compromise between realism and anti-realism, but not, I think, the one broached here. Fine sees the realist and the anti-realist as both "accepting the results of science." The realist augments this core position with a substantive theory of truth as correspondence, whereas the anti-realist goes beyond the core with a reductive analysis of truth, or in some other way.

Fine's idea is to retain the core and reject both sorts of proposals for augmenting it. In contrast, the opposition between empiricism and realism described in the present paper does not concern the notion of truth. What is more, the realism and empiricism with which I am concerned do not in any univocal sense 'accept the results of science,' since realism claims that these results include discriminations between empirically equivalent theories, whereas the empiricist denies this.

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